P.S. Schmid & Kauffmann mention having received three S. paratyphi A, O-forms from you. I had no idea you had these. Sometime would you send them? No hurry.

J.

P.S.

May 7, 1953

Dear Phil:

Yours of the 4th just received. I was hoping you would tear into the ms. It should be made as clear as possible, and my past performance in this direction has not been (to say the least) consistently exemplary.

It would obviously be a good deal easier for us to go over this thing in person. But I will not implore you again (won't I though!) to consider a brief detour to Madison, during your coming trip.

I will wait to hear your detailed remarks before discussing the ms. further. But I will try to answer some questions of fact. SW-959 seems to be my oversight, and of course has to be documented. It is your Hines V.A.H.

I do not think further absorptions are indicated now; the point is already adequately made.

You mention SW-986 as being diphasic. Does this mean you have separated out pure i and pure enx reacting colonies?

I purposely tried to leave out any detailed theoretical discussion, because I do not feel that any comprehensive theory of phase variation has yet been substantiated. I would have no objection to repeating in the discussion that diphasic types have been found in transductions to certain monophasics, as already stated on p. 13 and in table 2. There is no theoretical contradiction from this finding.

The back-biting behavior of the phage released from S. typhimurium LT-7 is perplexing to us too. There have been a few cases like it in the literature but none (including this one) have been carefully worked out. I would assume that LT-7 typically carries a phage which could probably be tested for easily enough on other indicators, but that only a rare particle has "mutated" so as to be able to attack the strain of origin. Treatment with penicillin induces the release of enough of the phage that there will be an appreciable likelihood for the occurrence of one of these mutant particles, which is then able to grow at the expense of the parent bacteria. There is something rather like this in K-12 (p. 58 of our Genetics, 1953 paper), but the lambda-2 mutation occurs much less frequently. Although I would assume that the typical phage released by LT-7 could be used for transduction if grown on a suitable indicator, we have in fact worked only with PLT-7' (the back-biting mutant phage) which can be grown either on LT-2 or on LT-7. Phage very definitely will transduce to bacteria which it will not lyse (e.g. PLT-22 on LA-22), but we have generally had other indicator strains to measure the phage by plaque formation. Without such strains, we probably could not grow the phage to sufficient titer, though we could perhaps use ultraviolet induction of lysis of lysogenic bacteria (Lwoff effect).

The lw situation would be nice to work out. Unfortunately the unique strains of S. dar-es-salaam and S. wien, which are about all there is to work with in